Melbourne
Sept. 25, 1957

Dear Jackie:

I'm still pondering over the significance of the letters just received from you, and from Ann, but before you do anything too desperate, I thought I'd better write. I wouldn't want to have your descent into sin on my conscience.

I agree that Pardee's conditions are quite different from ours, and that he doesn't have anything like the protection we get in a more complex medium. We don't know all the ins and outs of it, but it is quite clear there is more even to sucrose than its osmotic effect. But the point is that even with less protective conditions, he still gets continued incorporation of labelled glucose into the cell wall.

I had heard of this before, andin fact had a good talk with Pardee at Berkeley on our way through. I see no reason we should not accept this conclusion at its face value: the penicillin lesion (at least at lower concentrations) does not impair the assimilation of carbohydrate into wall components. Nor, according to Kandler, does it interrupt the incorporation of DAP. All well and good, we have to find some other aspect of wall formation that is interrupted. While Pardee does minimize this, he can hardly exclude it, and his alternative hypotheses are too bizarre to be worth considering. I would wish that he would study higher concentrations of penicillin: if these did interrupt the polysaccharide polymerization as well, it might help to understandthe zone effect.

His statement that the walls looked fairly normal is not too surprising, considering the rather limited amount of unbalanced growth, in his expts. The more reason for you to perfect your EM technique so you can compare fresh protoplasts with passaged L colonies.

The work you just wrote about is quite helpful in giving us an orientation on Pardee's work and its bearing on our own. However, unless we were prepared and equipped to go into the isotope-labelling business (which we should be but aren't at present) I don't think we can pursue this particular line much further. I am waiting to hear how the new L mutants are coming along, as well as some of the other lines pending. Have you got any S^r DAP^- mutants yet, to test the mechanism of streptomycin-resistance? Any more thin sections this summer?

To Ann:

1. There should be quite a large box with key-sort cards lying about. It might be on a high shelf behind the hood, or elsewhere around the lab; also look at the high shelves in Jim's lab and washup room, but I do think it's behind the hood pretending to be Petri dishes or the like.

2. Probably no point in looking for more rhamnose mutants if all these are of independent origin.

3. I can't imagine what W1366 is off hand. If there's an ambi-

3. Without my notes or stockbook, I can't help you about W1366. Is it mentioned in any of the papers, especially Esther's on Lac allelism? or does it have any other derivatives that would be a clue? If there is any ambiguity, it would be safer to go back to a reliable progenitor, rather than reisolate it now from a dubious mixture. At the very least, check the Lac character against the testers.

4. No, I can't make good sense out of the double reactions, especially with other segregants falling into line. Your figures on H-435 and H-436 would make it appear that these are not segregating la/lb at all, but some other Lac(s). This again should be checked for the parental input. It might also help to check the Lacs against other indicators, especially the 'deficiency' types. Let me know about this, particularly for the odd 'both pap.' segregants of H433.

Just conceivably, a Lac la/lb trans heterozygote might be so stabilized that it gave a definite - phenotype, but could recombine with either the la or lb tester. But this is remote. However, one could ask about the stability of such clones with regard to this reaction.

It would help me if you gave me more detail on the way the tests are done. But since some of the anomalous Lac$^-$ are prototrophic already, you should be able to pick up inherent revertibility without any trouble at all. I assume you cross-brush in such a way that you compare the spontaneous Lac$^+$ with those arising by recombination.

oOo

We're in the middle of a number of crosses of inluenza A virus. Most of it is going over old ground, but I have some leads on some more efficient selective markers and am looking for certain recombinant classes that have been missing in the work here so far.

oOo

I'm glad to hear the lab's in good order: can I really believe it?

I have been getting a couple of letters that were forwarded surface mail as they carried insufficient postage. No harm now, but it would make a mess later on. The only letters that can be forwarded directly are foreign aerograms or airletters marked '5 gms.' or the like. Other mail, including domestic airmail, has to get additional postage or, preferably, forwarded in the weekly packet.

Would you be kind enough to give the enclosed requisition to Bette?

Best wishes,

Joshua Lederberg